(presented by the Observatory); Portrait of Bishop Brinkley (presented by Sir R. Ball); Portrait of Manuel Johnson (presented by Dr. Rambaut);

And, in addition to the above, a series of photographs presented by the Director of the Yerkes Observatory which had been shown at the annual meeting—viz. 11 transparencies from Professor Hale's spectroheliographs of the solar surface, and 39 paper prints from photographs of stellar spectra, the Sun, instruments, &c.

Note on the Instrumental Errors affecting Observations of the Moon. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. In the November number of the *Notices* I described a method of photographing the Moon among the stars as a way of obtaining its place which seemed to be freer than the methods now in use from certain systematic errors, and the following sentence occurs in the first paragraph :—

> It was to obtain observations of the Moon in the first and last quarters that Airy set up the altazimuth at Greenwich in 1847; and unfortunately the instrument did not satisfactorily solve the problem.

At the time of writing these words it occurred to me that the evidence for or against the value of the altazimuth observations had never, so far as I know, been definitely published. Disparaging criticisms of a general nature have been made more than once, and were at any rate so familiar to me that the above sentence suggested itself naturally. But at the same time, for my own satisfaction, it seemed desirable to make some investigation of the behaviour of the instrument; and as an involuntary pause in our work for the Astrographic Catalogue set a computer free, an inquiry was started.

2. Soon afterwards I learnt that Mr. Cowell was at work on the Greenwich lunar observations, and it seemed probable that we were merely duplicating his work. But in reply to a definite question I learnt that he was not discussing the altazimuth observations at all—perhaps another illustration of the general disrepute into which they have fallen. Accordingly the inquiry was not interrupted; and it now seems fortunate that it was started, for, unless I am mistaken, the results indicate that the instrumental errors of the transit circle observations may be much larger than Mr. Cowell appears to have allowed for, and are liable to modify some of the deductions which he has extracted with such skill from the *Greenwich Observations*. In view of the fact that he is contemplating further work of the same kind, it seems important to draw attention at once to the possibilities.

3. In the Appendix to vol. l. of the *Monthly Notices* the Greenwich observations, with both transit circle and altazimuth, from 1847 to 1861 are compared with Hansen's Tables. In discussing these observations from the point of view of theory, it is important to make certain corrections to the tables, as Mr. Cowell has done (see *Monthly Notices*, lxiv. p. 85); but my immediate concern was with the difference between transit circle and altazimuth observations—both made practically at the same time and compared with the same tables, so that any error of the tables does not matter, and was neglected. The only *instrumental* inequality of the Moon is that which runs its period with the lunation, though the season of the year must also be taken into account. Thus the observations were grouped according to the day of the lunation, counting full moon as 15; and all the lunations with full moon in January were taken together; all those with full moon in February, and so on. Blank days were filled in by simple interpolation between the dates which had observations. Sometimes this process had to be carried to an extent which might be condemned; but it does not seem likely that the general results shown below can be essentially incorrect. Then the means were taken for all the January lunations for each day, then for all the February lunations, and so on. Finally, taking the mean value for the five days 13, 14, 15, 16, 17 near full moon in January, this was subtracted from all the January days; the mean for five days near full moon in February from all the February days, and so on.

4. Assuming the tables correct, we should then have the *instrumental* error for each day compared with that at full moon, for both transit circle and altazimuth, the observations with the two instruments being of course kept separate. If the tables are not correct, the tabular errors will be common to both series; and the *differences* should give merely the differences of instrumental errors.

5. Now what should these differences be? Before full moon the first limb is observed with both instruments; after full moon the second limb.* The exceptions are confined entirely to the three days numbered 14, 15, 16, and are so few that they may be probably neglected for the purposes of the present inquiry. We should be prepared for a different apparent diameter for the two instruments, which would mean a constant difference between them, changing sign at day 15. This corresponds to the assumption made by Mr. Cowell for the transit circle observations. He says (*Monthly Notices*, lxiv. p. 96):—

The hypothesis that the error of semi-diameter is a constant

* It might at first sight appear that a complication is introduced into the altazimuth observations, because the error in longitude is deduced from errors in azimuth and Z.D.; and in Z.D. sometimes the upper and sometimes the lower limb is observed. But on the hypothesis of a simple error in semidiameter, the effect on the longitude will in all cases be reversed after full moon.

is the best that I can make. . . . The mean correction for different ages of the Moon is no doubt a function of the age of the Moon and not a constant. If there were any means of obtaining its numerical values they could be analysed into the form

$$f(\mathrm{D}) + \alpha \sin \mathrm{D} + \beta \sin 2\mathrm{D},$$

when α, β would be corrections applicable to the results of this paper. . . . I do not think that α can possibly be as large as $0''\cdot30$, and it is possibly a good deal smaller.

6. From the point of view of the paragraph just quoted the comparison of the transit circle and altazimuth observations throws light on the possible values of α and β, though it does not determine them. If α and β are small for the transit circle they should be smaller for the altazimuth, since the observations made with the latter are made far more often in a dark sky. If the differences mentioned in § 4 are *not* constant we cannot feel so sure that α and β are small.

7. These differences are shown in the following table, wherein the separate results for both altazimuth and transit circle are also given, though these are made up of instrumental errors combined with errors of theory in a manner which need not concern us at present. In the first instance the means of all the months have been taken.

Table I.

Mean Error in Longitude of the Moon's Centre as compared with that at full Moon for all seasons.

Day of Luna-tion.	Altaz. 1847–1850.	Altaz. 1847–1861.	T.C. 1847–1861.	T.C.–A. Uncorr.	T.C.–A. Corr. I.	T.C.–A. Corr. II.
		″	″			
2	...	+2·1	...	...	...	...
3	...	+1·2	...	...	...	...
4	...	−0·1	−0·2	−0·1	+0·9	+0·7
5	−0·1	−0·3	−1·2	−0·9	+0·1	−0·1
6	−0·5	−0·5	−2·2	−1·7	−0·7	−0·9
7	+0·1	−0·9	−2·3	−1·4	−0·4	−0·6
8	−1·2	−1·2	−2·3	−1·1	−0·1	−0·3
9	−1·7	−1·2	−2·0	−0·8	+0·2	0·0
10	−1·4	−1·1	−1·9	−0·8	+0·2	0·0
11	−1·4	−1·2	−1·8	−0·6	+0·4	+0·2
12	−0·5	−1·0	−1·7	−0·7	+0·3	+0·1
13	−0·3	−0·5	−1·4	−0·9	+0·1	−0·1
14	0·0	−0·1	−0·9	−0·8	+0·2	0·0
15	+0·5	+0·4	0·0	−0·4	...	...
16	−0·8	−0·2	+0·9	+1·1	−0·2	0·0

Day of Lunation.	Altaz. 1847–1850.	Altaz. 1847–1861.	T.C. 1847–1861.	T.C.–A. Uncorr.	Corr. I.	Corr. II.
		″	″			
17	+0·5	+0·4	+1·4	+1·0	−0·3	−0·1
18	+0·3	+0·5	+1·8	+1·3	0·0	+0·2
19	+0·5	+0·5	+1·9	+1·4	+0·1	+0·3
20	+0·9	+0·7	+1·5	+0·8	−0·5	−0·3
21	+0·6	+0·9	+2·3	+1·4	+0·1	+0·3
22	+1·4	+1·1	+2·5	+1·4	+0·1	+0·3
23	+0·8	+1·0	+2·4	+1·4	+0·1	+0·3
24	+0·7	+0·8	+2·2	+1·4	+0·1	+0·3
25	−0·5	−0·2	+1·7	+1·9	+0·6	+0·8
26	...	−0·5	...	...	...	...
27	...	−0·2	...	...	...	...

8. In the first column is given the day of the lunation counting full moon in all cases as 15; to the second column reference will be made later in the paper; in the next two columns the mean errors with the different instruments. In the fifth column the simple difference is shown. Now, on the hypothesis that a different value of the semi-diameter is applicable to the two instruments, *and that only*, we may take the mean of the differences before full moon and the mean of those after to determine this difference. On the other hand, if we allow for a progressive change as well, we may establish continuity by taking the means of days close to full moon only—say three days before and three days after. The means on these two suppositions are as follows:

	Hypothesis I. 10 days before.	Hypothesis I. 10 days after.	Hypothesis II. 3 days before.	Hypothesis II. 3 days after.
TC−A	−1·0	+1·3	−0·8	+1·1

9. We notice that on either hypothesis there is a sensible divergence (0″·3) from symmetry. No probable explanation of this difference occurs to me at present, but it is worthy of note that we here have a curious and unexpected instrumental difference half as large as the value allowed as possible by Mr. Cowell for α, as quoted in § 5.

10. Applying these corrections for semi-diameter we get the columns shown as corr.$_{\text{I}}$ and corr.$_{\text{II}}$ Whichever column we take it seems improbable that the value of α can be so small as 0″·3; and we are here dealing with the *difference* between two instruments. The effect for either may be much larger. On the other hand it is possible that the whole error is in the altazimuth observations, and the transit circle observations are comparatively correct. But this should not be assumed without good reason being shown; for, as already remarked, the alt-

azimuth observations, rough though they may be, are made under more constant conditions than the meridian observations.

11. To check the above results it was determined to collect the information in an independent manner as follows. Only those days were included when there was both a T.C. and an altazimuth observation, and the direct difference of the errors in longitude was formed and tabulated under the day of the lunation, as before. Any error of the tables was thus eliminated by a more direct process, and the necessity of filling in blank days was avoided. Further, the observations made with the present transit circle, set up in 1851, were kept separate from those of the old one* used from 1847 to 1850. By an oversight this was not done in the previous work.

12. Let us first consider the comparisons on the hypothesis that there is simply a diameter peculiar to each instrument. Taking the means of all the comparisons before and after full, and taking groups corresponding to different seasons, we have

TABLE II. (Hypothesis I.)

	Old T.C.—Altaz. (1847–1850). Before Full.	After Full.	New T.C.—Altaz. (1851–1861). Before Full.	After Full.
	$''$	$''$	$''$	$''$
Nov., Dec., Jan.	−1·638	+1·848	−0·7	+2·2
Feb., Mar., Apr.	−1·935	+0·730	−1·7	+2·6
May, June, July	−1·967	+0·145	−0·4	+2·3
Aug., Sept., Oct.	−1·353	+0·759	−1·7	+2·3
Mean	−1·6	+0·8	−1·1	+2·4

13. We see that the curious asymmetry is much accentuated now that the comparison is made in this new way. Further, that it is quite different in character for the two periods 1847–1850 and 1851–1861. The mean error is negative for the former period (−0″·4) and positive for the latter (+0″·6); which seems to imply that, unless there was a corresponding change in the altazimuth observations themselves, the mean places of the Moon found by the new transit circle differ systematically by +1″·0 in longitude from those found by the older instrument. It is of course quite possible that the change was in the altazimuth and due to

(*a*) *Instrumental causes*, such as the introduction of the chronographic method of registration. Even such changes as are mentioned in the volume for 1851, viz. a change in the method of illuminating the divisions or of mounting the object glass, may be responsible.

(*b*) *Personal.* A remarkable personality in the case of Mr. Hugh Breen was detected almost as soon as the altazimuth was erected. That observer had a lunar personality (applying to both limbs of the Moon) of $0^s·31$. But this was detected in 1848 and the corrections made for existing observations, and Mr. Breen

* The term "old T.C." is used for brevity to denote the combination of transit instrument and mural-circle which was in use before 1851.

ceased forthwith to observe with the instrument. We may infer that such personalities were looked for with some care and probably eliminated; but some may have been overlooked.

But in either case, if the change was really in the altazimuth observations, it ought to be possible to prove this and free the transit circle observations from the suspicion of such a grave discontinuity.

14. To compare the results of Table II. with those previously found from Table I. we must combine them with the approximate weights 1 to 3; since the first series extended over three and a half years only and the second over eleven. We should then get $-1''\cdot2$ before full and $+2''\cdot0$ after, as compared with $-1''\cdot0$ before and $+1''\cdot3$ after (§ 8, Hypothesis I.) Our first results were therefore numerically too small as compared with the second. I feel tolerably sure that this is due to the way in which the first results are affected by the imperfection of the record, though I have not been able to establish this point completely. One way in which this imperfection tends to diminish these numbers is as follows:—Blank days were filled up by simple interpolation. Now, suppose we had observations on days 12 and 16. The former is subject to a correction $+1''\cdot2$, say, and the latter to a correction $-2''\cdot0$. On interpolation for day 13 we get a result subject to a correction $+1''\cdot2-\frac{1}{4}(3''\cdot2)$ instead of $+1''\cdot2$, as it should be; *i.e.* $0''\cdot8$ too small, and for day 14 $+1''\cdot2-\frac{1}{3}(3''\cdot2)$, or $1''\cdot1$ too small. Any days near full moon are thus liable to dilution in this way. In fact the process of §§ 7–10 is sensibly defective for our present purpose, and is to be regarded as superseded by the more direct method now being considered. The disadvantages of the present method, which prevented its use in the first instance, are of course (1) that a good many observations are lost; but this is not serious; (2) that we get no information as to the behaviour of the altazimuth in the part of the lunation where it is specially valuable, *i.e.* when the transit circle observations fail.

15. If now, instead of including *all* the results "before full," we limit ourselves to the three days, 12, 13, 14, as in Hypothesis II. of § 8, and similarly for "after full," we get

Table III. (Hypothesis II.)

	Old T.C.—Altaz. (1847–1850).		New T.C.—Altaz. (1851–1861).	
	Before Full.	After Full.	Before Full.	After Full.
Nov., Dec., Jan....	$-1\cdot7_{13}$	$+2\cdot5_{15}$	$-0''\cdot5$	$+0''\cdot9$
Feb., Mar., Apr.	$-2\cdot5_{14}$	$+1\cdot5_{11}$	$-0\cdot7$	$+2\cdot1$
May, June, July	$-0\cdot8_{26}$	$+0\cdot5_{17}$	$-0\cdot7$	$+1\cdot3$
Aug., Sept., Oct.	$-0\cdot8_{21}$	$+0\cdot7_{23}$	$-1\cdot6$	$+2\cdot2$
Simple Mean	$-1\cdot4$	$+1\cdot3$	$-0\cdot9$	$+1\cdot6$

16. Inspection of Tables II. and III. suggests that the effect of the seasons is comparatively small. We will therefore, in the first instance, disregard it and take all the observations through-

out the year together, as was done in Table I. The results are given in Table IV., which corresponds with the arrangement of Table I., but is to be taken as superseding it for reasons already given.

TABLE IV.

Differences between Meridian and Altazimuth Observations of the Moon's Longitude.

Day of Lunation.	Old T.C. – Altaz. (1847–1850).			New T.C. – Altaz. (1851–1861).		
	Uncorr.	Corr. I.	Corr. II.	Uncorr.	Corr. I.	Corr. II.
	″	″	″	″	″	″
5	$-4\cdot2_{8}$	−2·6	−2·8	−0·4	+0·7	+0·5
6	$-2\cdot6_{16}$	−1·0	−1·2	−2·0	−0·9	−1·1
7	$-3\cdot2_{14}$	−1·6	−1·8	−1·6	−0·5	−0·7
8	$-1\cdot9_{14}$	−0·3	−0·5	−1·8	−0·7	−0·9
9	$-0\cdot4_{21}$	+1·2	+1·0	−1·5	−0·4	−0·6
10	$-1\cdot1_{21}$	+0·5	+0·3	−0·9	+0·2	0·0
11	$-1\cdot8_{24}$	−0·2	−0·4	−0·6	+0·5	+0·3
12	$-2\cdot1_{22}$	−0·5	−0·7	−0·8	+0·3	+0·1
13	$-0\cdot1_{27}$	+1·5	+1·3	−0·8	+0·3	+0·1
14	$-1\cdot8_{25}$	−0·2	−0·4	−1·0	+0·1	−0·1
15	$-1\cdot0_{25}$	...	...	−0·5	...	...
16	$+0\cdot9_{21}$	+0·1	−0·4	+1·7	−0·7	+0·1
17	$+0\cdot4_{26}$	−0·4	−0·9	+1·5	−0·9	−0·1
18	$+2\cdot8_{18}$	+2·0	+1·5	+1·8	−0·6	+0·2
19	$+0\cdot4_{19}$	−0·4	−0·9	+2·7	+0·3	+1·1
20	$-0\cdot5_{18}$	−1·3	−1·8	+2·1	−0·3	+0·5
21	$+0\cdot9_{26}$	+0·1	−0·4	+2·3	−0·1	+0·7
22	$+0\cdot4_{22}$	−0·4	−0·9	+2·7	+0·3	+1·1
23	$+0\cdot9_{15}$	+0·1	−0·4	+2·6	+0·2	+1·0
24	$+0\cdot3_{10}$	−0·5	−1·0	+2·0	−0·4	+0·4
25	$-2\cdot5_{4}$	−3·3	−3·8	+4·4	+2·0	+2·8

17. The comparison with the old T.C. is affected with much larger accidental errors, owing to the smaller number of observations. To reduce the accidental errors a little let us assume that the errors are repeated with reversed sign after full moon. Hence we may reverse the sign of one set—say those before full moon—and take the mean. Further, we may smooth the results by taking means of every consecutive three. It will be sufficient to take the columns headed Corr. II.

TABLE V

Day.	Means of Before and After Old.	New.	Smoothed. Old.	New.	N−O.
	″	″	″	″	″
16	0·0	+0·1	...	...	...
17	−1·1	−0·1	0·0	0·0	0·0
18	+1·1	0·0	−0·1	+0·1	+0·2
19	−0·2	+0·4	−0·1	+0·2	+0·3
20	−1·1	+0·2	−0·7	+0·4	+1·1
21	−0·7	+0·6	−0·7	+0·6	+1·3
22	−0·2	+1·0	−0·1	+0·8	+0·9
23	+0·7	+0·8	+0·2	+0·9	+0·7
24	+0·1	+0·8	+0·1	+0·9	+0·8
25	−0·5	+1·2	...	...	...

18. These results are striking. They show, either (*a*) that the old transit circle and the new would give very different coefficients for the parallactic inequality; or (*b*) that the altazimuth was capable of behaving quite differently in the years 1847–1850, and in the years 1851–1861; that is to say, *the same* instrument was capable of assigning two very different values to this inequality.

In either case it seems to be necessary to make a careful study of the instrumental errors before deducing a value for the solar parallax entitled to any weight at all.

19. It throws some light on the question whether the altazimuth observations are at fault to recur to the results tabulated in Table I. and separate the group for the dates 1847–1850. The defects of these figures have already been noticed, but they chiefly concern the observations near full moon, and, further, they would be practically common to the whole series. Hence the second column of Table I. was prepared, and by comparing it with the next column it will be seen that there is a fairly strong presumption against any essential change in the altazimuth observations.

Summary.

(*a*) During the years 1847–61 observations of the Moon were made at Greenwich with three different instruments, viz. the altazimuth for the whole period, the present transit circle from 1851 to 1861, and the old transit and mural circle from 1847 to 1850. The altazimuth observations can thus be compared with those made with both the other instruments. The comparison is made through the medium of Hansen's tables, from the columns H—O in the Appendix to *Monthly Notices*, vol. l.

(*b*) The object of the comparison is to discuss *instrumental* errors, and not errors of theory; but it will show how far certain

terms in the lunar theory, especially the parallactic inequality, are affected by instrumental errors.

(*c*) The Moon's longitude determined by the old meridian instruments differs from that by the altazimuth, *after allowing for different apparent diameter*, by a *variable* quantity which rises to $-0''\cdot 7$ at about the quarters compared with full moon, and is represented in the fourth column of Table V.

(*d*) The new transit circle, on the other hand, shows an increasing positive difference, slowly increasing even past the quarters (see the fifth column of Table V.)

(*e*) The evidence is against this change being due to any change in the altazimuth.

(*f*) It therefore seems probable that meridian instruments may give very different values for the parallactic inequality ; and the results derived by Mr. Cowell on pp. 95–98 of *Monthly Notices*, vol. lxiv., especially that for the Sun's parallax, must, until these instrumental errors have been more fully discussed, be received with caution.

University Observatory, Oxford:
1904 *February* 20.

Methods of Analysis of Moon's Errors and some Results.
By P. H. Cowell, M.A.

I have had a very short glance at Mr. Nevill's paper * in MS. before it was sent to the printers, and in consequence I am now giving a full account of my methods, as my recollection is that Mr. Nevill misunderstands me in a few points. I gladly take the opportunity of acknowledging the assistance that Mr. Nevill's paper is sure to be to me, and I must also admit two or three errors in my paper that Mr. Nevill points out. Nothing in my investigations at present has turned upon these errors, but it is naturally of service to me to have them pointed out. I may add that as regards the term in cos ☊ I followed Airy too rashly. He expressly states that this term is not required by theory.

My method briefly is to analyse the observed errors. I naturally use the theory to suggest periods, and as far as possible to interpret the results, but my method is essentially empirical. I hope to obtain from the observations all inequalities in the errors of Hansen's tables exceeding $0''\cdot 5$, except, perhaps, in a few instances of terms of very long period, where such accuracy is probably unattainable. A term of less than $0''\cdot 5$ will probably not be detected, but coefficients of terms examined should in

* It has been found necessary to send the proofs of Mr. Nevill's paper to the author for correction. The paper cannot therefore appear in the present Number. Professor Brown's paper is also deferred for a similar reason.